# SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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FRIDAY, JULY 3, 1903.

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## *J. PETER LESLEY.*

PROFESSOR J. PETER LESLEY, born in Philadelphia, Pa., September 17, 1819, died in Milton, Mass., June 1, 1903.

After graduation at the University of Pennsylvania in 1838, J. P. Lesley served as aid for a year on the First Geological Survey of Pennsylvania. In 1840 he was assigned to independent work in the complicated northeastern area for several months, after which he was associated with Mr. James T. Hodge in the coal region of the southwestern counties. During the next year he made a reconnaissance of the coal deposits in western Pennsylvania and closed the season's work with a revision of Whelpley's studies in the anthracite region.

The abrupt ending of the survey in 1841 scattered the assistants, and Lesley went to Princeton Seminary, where, to use his own words, he 'indulged in the luxury of a course in theology.' But while studying theology he had no opportunity to neglect geology; his skill as a geological draughtsman and his familiarity with the conditions in a great part of Pennsylvania made him indispensable to Professor H. D. Rogers, who was striving to secure publication of the final report. Every hour which could be spared during term time and the whole of the vacations of 1842 and 1843 were devoted to preparation of the Penn-

sylvania geological map, to reduction of vertical sections to a uniform scale and to construction of cross-sections.

Having completed his theological course, Lesley was licensed to preach in 1844 by the Presbytery of Philadelphia and at once went to Europe, where he made a pedestrian tour through France and Germany, which he rounded out with a brief course of study at the University of Halle. Returning to America he undertook colportage work in northern Pennsylvania for the American Tract Society, which he pursued with characteristic energy and success for two years. In December, 1846, Professor Rogers asked him to come to Boston, where for five months he prepared duplicates of the state map and of the geological sections, which were to be deposited in the State Capitol at Harrisburg. While in Boston he received and accepted a call to the pastorate of the Congregational church at Milton, Mass., where he remained until 1851. In this interval his views respecting some theological questions developed along lines not wholly acceptable to his ministerial associates, so that at the end of four years he resigned his charge, abandoned the ministry and returned to Philadelphia, where he began practice as a consulting geologist. At once his services were sought again by Professor Rogers, who had obtained an appropriation for publication of the final report, and for more than a year he was engaged upon revision for that report.

Thenceforward for forty years his labor was incessant; there seemed to be no limit to his capacity for work. He was recognized at once as the most competent of geological experts and his time was fully retained. Yet from 1855 to 1859 he was secretary of the American Iron Association, for which he published in 1859 a huge volume, the 'American Manufacturers' Guide,' a remarkable compendium of theory, practice and statistics, which even now is of great value. For twenty-seven years he was secretary and librarian of the American Philosophical Society, rarely absent from meetings and seldom failing to present a paper or to take part in the discussions. He made elaborate surveys of the Cape Breton coal field, of the Pennsylvania Coke region, of the Cumberland Valley iron ores, of the Tennessee coal area, of the North Carolina iron ores; while he found abundance of time to learn several languages and to prosecute special studies in various departments of literature and philosophy. In 1872 he was made professor of geology and dean of the faculty of science in the University of Pennsylvania; but in 1878, owing to the pressure of other duties, he resigned the deanship. The Second Geological Survey of Pennsylvania was authorized in 1874, and he was placed in charge of the work. This post he retained until 1893, when sudden and complete failure of health compelled him to relinquish it. He retired to Milton, where he remained until his death.

His labor was unremitting during the twenty years of service upon this survey. He read the report of every assistant and prepared most of the admirable indices which make those reports so available; in many cases he drew the base for the maps and sometimes even transferred the outcrop lines from manuscript sent in by the field assistants. He maintained that there was no other way by which he could acquire complete mastery of the facts contained in the reports. He wrote long prefaces to most of the volumes, discussing the results, and in several cases he rewrote reports that the matter might be presented in a more systematic way. These prefaces and editorial notes did not always seem to the authors to be either necessary or val-

uable, yet, after a score of years, it must be conceded that not a few of the suggestions, which were most unsatisfactory at the time, have proved to be of lasting value. As if these occupations were not enough, he made frequent field studies, delivered many addresses and lectured to the college classes. Such unceasing toil told even on his extraordinary constitution; several times he was compelled to abandon everything abruptly at the close of the winter's work and to flee to Switzerland, where, with Desor and other friends, he would spend two months of absolute freedom from all care—but only to return to work at the same terrific pace, to make ready for another collapse.

The hundred volumes of reports giving the results of the Second Survey are his monument. He gathered around him a group of earnest workers into whom his own spirit was infused; in most instances he gave them free scope and was repaid by honest investigation. At the close of the survey work he undertook to prepare a final report; but the close application, which he deemed necessary, brought on the final break after he had completed the report up to the end of the Lower Carboniferous. In this marvelous compilation he gave a synopsis of every assistant's work, according unreserved recognition to each observer and frequently showing an unselfish neglect of credit due to himself for earlier discovery of facts and determination of principles.

Keen in perception, quick in comprehension, Professor Lesley at times reached conclusions too hastily, but no man was quicker than he to acknowledge an error. His broad reading and tenacious memory made him a well-furnished scholar; his cheery disposition made him an attractive companion. He knew little of the world and cared less for it; he was a typical student, who in worldly matters never outgrew his college days. Honest and true, he never remembered an injury, he never forgot a kindness. His faults were those of a whole-souled generous man.

For ten years Professor Lesley was laid aside from all labor, but he bore his affliction with more than patience and at last he passed away peacefully, without suffering, literally crossing the threshold in sleep.

In 1849 Professor Lesley married Susan I. Lyman, of Northampton, Mass., who, with two daughters, survives him.

JOHN J. STEVENSON.

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## *AN ASPECT OF MODERN PATHOLOGY.**

IT is a truism to assert that the great progress made in pathology during the past century is the result of the study of cellular structure and activity. The close of the nineteenth century has witnessed no lessening of the interest of pursuit of this study; but it has seen arise an endeavor to penetrate more deeply into the nature and properties of cells through which their manifold activities are brought about. Armed with a rich harvest of facts and methods supplied by physiological chemistry, investigators have attacked the question of the internal constitution of the cell with renewed vigor, and the degree of success of this effort is indicated by the strides made within the past two decades in unraveling the phenomena of immunity and allied states. The twentieth century has received from its predecessor a rich heritage of facts and principles relating to the intimate structure and function of cells, which is destined to yield a fruitage of great importance to physiology, pathology and practical medicine

I find myself in the enviable attitude of dealing with certain topics in experimental

* Read at the annual meeting of the Medical and Chirurgical Faculty of Maryland, April 24, 1903.

pathology, of recent acquisition and somewhat obscure nature, possessed by the assurance that my audience can afford to dispense with an explanatory introduction because of its acquaintance with the newer facts of immunity in relation to pathology, so brilliantly dealt with in Professor Welch's Huxley lecture.

So many pathological phenomena have, in the past, been attributed to alterations of the blood, and recent discovery has added so largely to the list of the activities of this fluid, that one is tempted, as he views the wonderful properties with which it is endowed, to exclaim with Goethe: 'Das Blut ist ein ganz besonderes Saft.' The manner of the solution of blood corpuscles brought about by alien serum is a well-known phenomenon that many years since, through the studies of Landois and others, was made to explain the unsuccessful and even disastrous effects of blood transfusion. More recent investigations have shown that by a process akin to artificial immunization to bacteria, a similar but more intense capacity of bringing about solution of blood corpuscles can be developed in blood serums which naturally do not possess this action, and increase of the power can be produced in serums in which it is already present.

This solution consists in the liberation of hæmoglobin from the stroma of red corpuscles and its diffusion through the fluid, a process to which the name 'hæmolysis' has been given. Careful observation of this phenomenon has shown that, in many cases, a state of coalescence of the corpuscles, to which the name 'agglutination' is applied, precedes that of solution; and, further, that while these changes are often associated, yet one may occur in the absence of the other.

While, under normal conditions, the serum of an animal is without injurious action upon the corpuscles suspended in it, yet in certain pathological states the serum suffers an alteration through which its corpuscles are acted upon injuriously, are made to agglutinate, and even to undergo complete dissolution. Coincident with the appearance of this activity, it often happens that the serum has acquired increased power of solution over corpuscles of a foreign nature; and this alteration in properties acquired by the serum is held responsible for the blood destruction that accompanies many of the infectious diseases, and is so apparent and serious a condition in certain diseases—*e. g.*, severe anæmias, the causation of which is still unknown.

In view of the acquisition of such new and hurtful qualities by the blood serum, the discovery in cultures of many kinds of pathogenic bacteria of hæmolytic and agglutinating substances for red corpuscles is a peculiarly welcome addition to our knowledge of blood destruction in disease. And while this knowledge has not led to the understanding of all forms of blood destruction, and has as yet failed to shed important light upon certain clinical types of severe and pernicious anæmia, we have obtained a new standpoint from which to view the so-called blood diseases that promises further progress in the near future.

It is common experience in science to find that accurate observation has preceded adequate explanation. The manner of action of bacterial agents upon red corpuscles, and our recently acquired knowledge of agglutination of cells in general, could not fail to suggest that certain kinds of thrombi are produced by a form of coalescence of corpuscles; and I was not, therefore, surprised to find unmistakable evidences of agglutination of red corpuscles in the blood vessels of the intestine and other organs in typhoid fever, in the lung in lobular pneumonia, and in some other

pathological conditions in man. That a similar form of thrombosis plays an important part in experimental pathology could be shown by studying the 'clots' formed in the heart and great vessels in poisoning by ether, alien serum and ricin injections in rabbits, and has been proved by the observations made by Fisher, working in Dr. Welch's laboratory, of the occurrence of such thrombi in experimental infections with the typhoid bacillus, and Boxmeyer, of Boston, of their existence in relation with certain necrotic foci in the liver in experimental hog-cholera bacillus infections. Heüter and Klebs and Welch had, many years ago, drawn attention to the fact that certain of the so-called hyaline thrombi appeared to be composed of fused and altered red corpuscles, observations which are confirmed and explained by these later findings.

It seems to me highly probable that agglutinative thrombosis will, in the near future, be recognized as an important pathological condition in man, and that a large number of the thrombi that arise in the course of infectious disease and, doubtless, diseases of uncertain etiology, will be discovered to have originated in agglutination. Not only will capillary hyaline thrombi be explained upon this basis, but the thrombi of larger vessels, such as those of the femoral vein, may equally be shown to have this mode of origin. I wish, in this connection, to draw attention to the unsatisfactory nature of the data relating to the mycotic origin of thrombi with which French writers especially have identified themselves, and to suggest that in agglutination a more adequate cause for thrombosis might come to be found.

The experimental study of agglutination seems capable of shedding much needed light upon the subject of intravascular clotting in general. I have been able to show that the so-called coagula which appear in the heart in experimental ether and alien serum poisoning are not clots in a true sense, but merely masses of fused red corpuscles; and Ehrlich many years ago endeavored to explain the lesions of experimental ricin poisoning upon the basis of his findings of capillary thrombi composed of agglutinated corpuscles. Ehrlich's attention was called to this appearance by the previous observations of Kobert and Stillmark upon the precipitating and coagulating effects of ricin upon red corpuscles *in vitro;* but the entire series of pathological effects of ricin upon the organism can not, as I pointed out some years ago, be explained by thrombosis.

That relation exists between extensive blood destruction and agglutinative thrombosis, the mode of action of ether and alien serum injections in producing rapid death quickly proves; and the essential identity of the agglutination produced *in vitro* and in the body by agglutinating and hæmolytic substances can be easily shown. If a dog or a rabbit is injected with a fatal quantity of hæmolytic serum and the gelatinized blood in the heart and great veins be removed and subjected to immediate examination in the fresh state, the corpuscles will be found fused together and, under the influence of the pressure of a cover-glass, to undergo extraordinary changes in form and position. Any one acquainted with the remarkable photographs of Mitchell and Reichert illustrating the effects of venom upon red corpuscles will recognize the identity of the two pictures.

The description of agglutination of corpuscles by venom given by Mitchell and Reichert, and later by Mitchell and Stewart, in their papers on venom, and the studies upon venom recently conducted by Dr. Noguchi and myself, suggested a re-

study of the intravascular clotting of blood caused by venom injections. We were fortunate in having several kinds of venom with which to undertake this investigation. It has long been recognized that the venom of the viper family, to which our rattlesnake belongs, is especially prone to cause vascular clotting. Besides crotalus venom we have possessed other viper venoms—such as that of Russell's viper from India (*Daboia Russellii*) and *Trimere surus* from Japan. By far the most active venom in this respect is that of Russell's viper, as intravenous injections of it cause almost instantaneous 'clotting' of the blood in the right heart, pulmonary artery and vena cavæ. If these clots are removed immediately and examined (1) in the fresh state, and (2) after instantaneous hardening, no fibrin can be found. They consist exclusively of masses of agglutinated red corpuscles and are entirely free from evidences of clotting in the classical sense. But besides the change in adhesion of the corpuscles, still greater alterations of form and refraction have taken place in them; and sections of the 'clot' from the heart and in the vessels of the lung show extraordinary pictures of drawn and twisted bands of hyaline appearance which may readily be mistaken for modified fibrin.

These facts would seem to be of immense assistance in explaining the origin of certain thrombi met with in man in infections and other diseases, which are attended by marked blood destruction, and perhaps still other changes in the composition of the blood.

The light which the study of agglutinines has shed upon the general subject of thrombosis has served also to illuminate in no trifling way the path along which we are proceeding in gaining knowledge of certain forms of hæmorrhage. It is customary to ascribe hæmorrhages to two opposed conditions—to rupture of blood vessels and escape of blood corpuscles through the vessels by diapedesis. Only the second mode of origin needs enlightenment. Hitherto we have been obliged to be satisfied with a vague and hypothetical molecular alteration of the vascular wall as explaining the increased passage of red corpuscles into the tissues. It would now seem that at least certain forms of parenchymatous hæmorrhage are explicable in a more satisfactory and objective manner; and I will ask you kindly to turn your attention to this subject.

That no necessary relation exists between thrombosis and hæmorrhage can be shown experimentally by removing from rattlesnake venom its agglutinines for red corpuscles, when the hæmorrhage principle is left unaffected; and that no relation exists between hæmolysis and hæmorrhage can be inferred from the action of cobra venom which, while an active agent of blood destruction, causes but little hæmorrhage. But the entire independence of the principles which act injuriously upon blood vessels, and thus permit escape of blood, can also be shown by the use of ricin, which is non-hæmolytic, and which, in rabbits, produces extensive extravasation of corpuscles into the serous membranes.

Jacoby and Müller have shown that ricin is not robbed of its entire toxicity by digestion with artificial gastric juice, and Müller made the observation that the agglutinine is destroyed in the process. I found no trouble in confirming this finding of Müller, and in detecting that digested ricin is still capable of producing hæmorrhage.

If the thin mesentery of the rabbit is spread out and prepared and the capillaries lying within the hæmorrhagic areas are carefully studied, it will be seen that in neither venom nor ricin poisoning have

the red cells become fused and lost their independence, but the capillary walls have been injured in a definite and unmistakable manner. The extravasations take place not by diapedesis, as is now believed, but through actual rents in the walls. The explanation of the rents is of much interest. That they are not simple ruptures seems proved by the disappearance, as if through solution, of the parts of the walls at the site of the escape of corpuscles. The solution of continuity is one-sided, and in some instances is attended by a displacement of the adjacent endothelial cells, which are pushed outward, away from the vessel, by the force of the escaping blood. The escape of corpuscles by dissolution of the vascular walls is limited to capillaries and small veins. When acted upon by venom the vessels show irregular bulging of the walls, by ricin a localized dilatation or congestion of the vessels, which give rise to a glomerular appearance; but in a small number of places only does the extravasation occur. It is probable, therefore, that the points of injury to the vascular coat are many, but in a part only of these does the vessel give entirely away.

The existence, then, of a substance having an especial affinity for vascular endothelium may be considered as proved for snake venom and ricin. For this principle, if principle it be, we have proposed the name of hæmorrhagin, and we look upon it as a cytolysin for endothelial cells of blood vessels, the injury and destruction of which is the direct cause of the escape of blood into the surrounding tissues. It remains to be shown whether such hæmorrhagins are of common occurrence in nature, and to what extent they may play a part in animal pathology. I regard this as a most promising field of future exploration, for I conceive that bacteria and other pathogenic microorganisms probably produce endotheliolysins, and the action of hæmorrhagins may be the immediate cause of the extravasation of blood in purpuric states and some other forms of so-called parenchymatous hæmorrhage.

Should this view of the causation of hæmorrhage be supported by future studies we may well consider the possibility of preparing experimentally an antidotal agent for the principle involved in its production. We already have at hand certain observations bearing upon this question. You are all familiar with antivenin as prepared by Calmette and Fraser as an antidote to venom. Although contrary to the generally prevalent opinion, it may be affirmed that the value of the commercial antivenin is in inverse ratio to the hæmorrhage-producing power of the venom. This fact arises from the consideration that in the preparation of antivenin, cobra venom chiefly is employed, and it is almost devoid of the hæmorrhagic principle.

Venoms which cause much hæmorrhage exert a very destructive local effect upon the tissues. For this reason very little success has attended the efforts to produce an anti-toxin for viper venom. On this account, Dr. Noguchi and I have sought a means of modifying viper venom so as to remove the locally destructive effect and yet leave the value of the hæmorrhagic principle with which an immunity might be established. Through the use of hydrochloric acid we have succeeded in getting rid of the local effects, including the hæmorrhage, and yet have preserved the combining value of the hæmorrhagic principle, so that successive injections in animals of modified venom could be carried out. The serum of these animals contains an appreciable quantity of anti-hæmorrhagin as well as other antidotal principles, and is capable of neutralizing the local effects of rattlesnake venom.

That anti-hæmorrhagins, like other antitoxins, etc., could be produced experimentally must be inferred from Ehrlich's production of anti-ricin; for although he failed to distinguish between the hæmorrhagic and agglutinating principles in ricin, yet the neutralizing value of his anti-ricin seems to have included all the ricin principles.

If I have dealt somewhat fully with this topic it is, first, because of its importance to human medicine, and second, because in its light I must regard the outlook for a better understanding of a very obscure, serious and difficult pathological condition in man as of considerable brightness. Moreover, it may not be an Utopian dream, in view of what has already been gained, to look forward to the production of an antidote that, by neutralizing this poison for vascular endothelium, may provide a rational and certain therapeutic agent to combat this form of hæmorrhage.

I shall ask you to turn to another aspect of my theme, which relates to the occurrence, under natural conditions, of a whole host of cell-destroying—cellulicidal—substances in blood serum which, up to now, have received almost no attention, and which I regard as not without significance to human pathology. When one considers the diversity of agglutinines and solvents for blood corpuscles, bacteria and other cells contained in normal serum or open to experimental production, it will cause no great surprise to learn that the serum of warm- and cold-blooded animals contains corresponding active principles for kidney, liver and testicular cells. Dr. Noguchi and I have been engaged, during the past winter, in studying these cytotoxins, and have found them to have a wide distribution and to possess a considerable degree of activity.

In view of the ever-increasing number of activities with which almost daily discovery is endowing our body-fluids, the question of the independence and specificity of their constituent active principles has come to be an important one. In the studies under consideration we could show, by means of absorption test, that the agglutinines and solvents for blood, kidney, liver and testicular cells differ among themselves, and the removal of a part of them by means of certain of the cells does not prevent the action of the serum upon the remaining cells; from which it could be concluded that these principles are at least specially adapted to given cells. A general reduction in the activity of the serum which has lost a part of the solvents also suggests that they are not specific in the strictest sense.

It would carry us too far afield to discuss the different ways in which the manifold properties of serum may be explained; whether by supposing it to be a mine of diverse substances as rich as the endless activities which it exhibits, or whether its effects are produced by combinations and permutations among a smaller number of independent bodies. It is sufficient for the moment to have drawn attention to the multiplicity of the energies of serum in a relatively narrow circuit, in order that I may add a word upon what may not be an impossible form of activity, developed under pathological conditions, within the serum.

In speaking of blood destruction, I emphasized the absence from serum under normal states of cytolysins directed against its own cells, and took under consideration the pathological conditions under which a destructive property was apparent. Whether similar harmful properties for the organic cells are developed in serum has not been considered especially, but in view of the diverse activities of bacterial and other poisonous agents, they may well be

assumed to arise. But just as not all the forms of blood injury can be ascribed to the action of exogenic poisons, it is worth while inquiring whether any conditions may arise under which the injurious power of serum may be directed against its bodily organic cells.

You will not have failed to appreciate the dangerous nature of the forces inherent in serum, but fortunately for us these implements of destruction are not turned against ourselves. The protection which the body exercises against these weapons of offense is aptly described by Ehrlich as 'horror autotoxicus'—horror of self-poisoning. Were it conceivable that the body should be withdrawn, for an appreciable interval of time, from the operation of this restraining force we might, at any moment, be observed to run together and dissolve in our own juices! And yet, is it wholly without the realm of possible accidents that in respect to some organs and in some degree this 'horror' should be removed? I can not convince myself that in the progressively degenerative lesions of the body —those of the liver, kidney and brain, for example—where through years the process of destruction goes on, and where the reserve regenerative capacity normally present is held in check, that this 'horror' may not be in abeyance.

The many observations upon the effects of iso- and hetero-lysins—as for kidney and liver cells—about which there is no reasonable doubt, speaks, it seems to me, in favor of such a possibility. Dr. Pearce has studied through many months, in my laboratory, the action of nephrolysins, and has made observations of great importance with reference to experimental nephritis. The results of his studies will appear in due time, but I wish to refer to one fact brought out by his investigations that I think of especial interest.

A large part of the studies were made upon dogs, and by the way of preliminary observation, the urine was always examined before the experiment was begun. It was surprising to discover not infrequently albumen and casts in the urine of dogs apparently in the best of health; and on studying the kidneys, to find marked degeneration of the epithelium, and focal accumulations of cells of the plasma-cell type, such as occur in man in a definite form of non-suppurative interstitial nephritis.

The blood serum of normal dogs infused into other healthy dogs produces no symptoms nor disturbance of the renal function. But the serum of a dog with spontaneous nephritis gave rise to albuminuria and cast excretion, such as Dr. Pearce has observed in many instances of the infusion of the serum of the rabbit which had been treated previously with washed dog's kidneys.

That both iso- and hetero-nephrolysins set up the lesions of acute nephritis had been shown previously; but this observation of Dr. Pearce is of a different order. It must be considered either that some exogenic toxic agent set up the renal lesions in the first dog, and was present in the animal's blood in such quantity that, when its serum was infused in the second animal in the proportion of 1 to 500 of the body weight, it sufficed to produce marked disturbance of the renal function, such as is recognized in man as due to organic lesions; or that the degeneration of epithelium which may be assumed to exist in the first animal (which is still alive and under observation) provoked a series of changes with the production of toxic substances which for this animal are autotoxic and another animal of the same species isotoxic. It will not do to dogmatize about phenomena as complex as those we are now considering; but if the second view expressed

here is at all tenable, and it would seem to offer at least a rational explanation of the facts observed, we must admit the possibility, under pathological conditions, of the establishment of a vicious circle leading to progressive degeneration of organs, which could come into play only by the temporary suspension of 'horror autotoxicus.'

But I must turn from such baneful and, at present, perhaps, unprofitable speculations. The whole subject to which they refer represents a field of future exploration. That it is a territory not without fascination you will, I think, admit; and I am of the opinion that it is also a land of promise for the future of practical medicine.

The enormous advances made in the last decades in the study of the morphology of the cell are being paralleled by the gains in our knowledge of what may be called intracellular chemistry. This new knowledge is bearing the richest fruits, for among them are the newer conceptions of immunity, and of the physiological and pathological activities of a whole series of intracellular ferments.

Looked upon broadly, the corner-stone of modern pathology is toxicology. Without entering into a discussion of the general subject, I may remind you that while definite chemical poisons, such as arsenic, morphia, strychnia, etc., are capable of inflicting great injury upon different organic cells, their introduction in repeated feebly toxic doses into the body is not followed by a reaction the results of which are the appearance in the blood and elsewhere in the body of neutralizing and antidotal substances. The case is wholly different with a series of less definite, chemically speaking, toxic agents of which diphtheria and tetanus toxins, ricin and venom, and pathogenic bacteria and other cells are examples, for when introduced into the living body in this manner, they give rise to antitoxic and destructive substances to which the names antitoxin and cytolysin are being applied.

The precise manner in which these antagonistic bodies come to be produced is, for the present, purely speculation. But the lateral chain hypothesis of Ehrlich, which attempts to supply a graphic conception of the manner of their formation, has, whether expressing the truth or not, led to great advances in our knowledge. According to this hypothesis, the antitoxins, intermediary bodies, agglutinines, etc., are yielded by certain constituents of cellular protoplasm within the body, designated 'lateral or side chains' or 'receptors,' which combine with the protoplasmic constituents of body cells, bacteria or toxins used for immunization. This conjunction seems to injure or render useless the receptors of the cellular protoplasm without, at the same time, so seriously damaging the cell as to prevent regeneration. The regenerative process does not exactly restore the integrity of the cellular protoplasm, but, in keeping with the general law of regeneration enunciated by Weigert, there tends to be formed similar bodies in excess. The excessive or lateral chains, being useless to the cells in which they are produced, are cast off and appear in the body juices as intermediary bodies or 'ceptors,' which, according to their nature, are designated uniceptors (antitoxins, etc.) and amboceptors (intermediary bodies).

In antitoxic neutralization direct union between the toxin and antitoxin occurs; while in bacteriolysis and other forms of cytolysis there is conclusive evidence that, although the intermediary body unites first with the cells, this substance by itself can not bring about injury or solution, but after its union with the cells the substance called 'complement,' normally present in

the blood, is capable of being brought into action, whence the injury is inflicted. The action of the complement depends upon its possession of properties designated zymotoxic and toxophoric, through the influence of which hæmoglobin is set free from red corpuscles, various organic cells are dissolved, bacteria are disintegrated, and ciliar and flagellar motions are suppressed.

The intermediary bodies and complements upon which serum activity depends are contained within the blood; but there are certain kinds of natural poisons, of which venom is perhaps the best example, in which only the intermediary body occurs in the poison, the complement—the directly hurtful constituent—being supplied by the blood. I have already dwelt upon some of the principles of this poison, and I wish now to state briefly that venom possesses intermediary bodies capable of bringing into play complements which cause solution of many kinds of cells—those contained in nervous, renal, hepatic tissues and still other organs. All these solvents, as Dr. Noguchi and I have been able to show, possess a striking independence of action which can not fail to excite great wonder at the complexity of venom, and aid in the understanding of the elective affinities which poisons exhibit for certain organs, to which not only is disease of these organs to be attributed, but the selective action of remedies ascribed.

My purpose in bringing again to your attention the subject of venom intoxication is to present a remarkable instance of interaction of two substances, both of which are poisonous, but one of which is capable of affording protection from the other; a therapeutic paradox which is explicable upon the basis of the mechanism of immunity as formulated by Ehrlich.

I have referred to the cytolytic action of snake serum, and I wish now to tell you that the blood serum of the rattlesnake, moccasin and some other snakes is highly poisonous to warm-blooded animals. Dr. Noguchi and I have been able to show that the manner of action of the toxic principles of snake serum is comparable to that of venom, with, however, one very important difference. Both owe their poisonous action to intermediary bodies; but the one, that of venom, is able to attach the complement, necessary to complete the injurious system, of the animal poisoned; while the other, that of serum, can combine only with its own complement. While, therefore, venom is active even after heating to a relatively high temperature, serum is inactivated at the temperature (58° C.) at which its complement is destroyed. This temperature does not, however, affect the serum-intermediary body; and hence, while the heated serum is no longer toxic because it can not utilize a foreign complement, it is still able to unite, through its intermediary body, with cells for which it has affinity. This affinity is especially for nerve cells; and since the serum combines with the same lateral chains or receptors of the nerve cells that venom attacks, it is possible by using heated serum to prevent a later venom union. If, therefore, heated snake serum is injected into guinea-pigs they can be protected, for a time, from fatal cobra poisoning. The duration of this immunity is not great and its degree is not high; for, on the one hand, the normal metabolism of the cell modifies or destroys, sooner or later, the combined serum-intermediary body, and, on the other, an excess of venom can by mass action drive out the weaker serum constituents. This fact is brought readily into conformity with the belief that receptors are, after all, designed primarily, not as organs to be used under stress of pathological necessity, but

to serve the needs of physiological processes of nutrition.

Snake serum protection from venom had previously been observed by Phisalix and Bertrand, who endeavored to explain the phenomenon by supposing that antivenin was produced by the absorption of venom into the circulation of those animals. According to their view, the serum contains venom and antivenin; the first easily destroyed by heat, the second more resistant. Heating of the serum, while abolishing the venom activity, leaves that of the antivenin unimpaired, whence its protective action.

There is something highly artificial in this explanation, which agrees, moreover, very badly with the facts. First, venom is destroyed at a much higher temperature than antivenin; second, the toxin and antitoxin of venom, when brought together, whether in the circulation or in a test-tube, tend to combine and neutralize each other; and third, it is possible by replacement experiments *in vitro* to demonstrate the occupation of the receptors by the neurotoxin of serum and the consequent exclusion of the neurotoxic constituent of venom.

You will, I trust, pardon me for bringing before you a subject of such purely theoretical interest. My object in doing so is to indicate the aid which the chemical idea of cell and toxine unions is bringing into toxicology. But I shall not need to apologize deeply for this liberty, since in it may be detected a purpose in pointing the way along which pharmacology and later therapeutics may be conceived to pass.

We have now traversed a considerable territory and one not wholly free from rough places. But the commanding position which we have reached, and the breadth of view into the nature of pathological processes which has been secured, justify, I trust, the undertaking. But having proceeded so far, it were vain to turn back until we have examined another field just opened up to physiological and pathological investigation, at the entrance of which we stand.

That organs protected from decomposition tend to undergo solution by a process of self-digestion was first accurately shown by Salkowsky in 1882. Within the last two years, and chiefly through the labors of Jacoby and Conradi, interest in this subject has been revived, and a number of important facts, bearing upon physiological and pathological processes discovered. Autolysis, as the process is now called, has been studied from many different sides; the nature and the distribution of the active ferments; their multiplicity and specificity; their influence upon the coagulability of the blood, upon bacterial life, upon cellular degeneration under physiological conditions, and upon the resolution and absorption of pathological products, formations and exudates. These studies have already shown that in the intracellular ferments causing autolysis we possess a most important and potent series of agents which come into play under both physiological and pathological conditions.

All that is required to convince oneself of the phenomenon of autolysis is to place a portion of the liver or other organ, or a quantity of exudate, under conditions protected from decomposition and maintained at blood heat, when digestion will proceed. Care must be exercised to avoid using means of preventing putrefaction that may injure unduly the ferments. Two kinds of autolysis are now distinguished: (*a*) Antiseptic, in which it proceeds under chloroform or toluol, and (*b*) aseptic, in which the organs are exposed in a sterile condition. Manifestly only the first method is applicable to certain pathological products in which bacteria are present. As a result of this self-digestion, the coagulable al-

bumens are converted into non-coagulable proteid, and certain by-products, among them leucin and tyrosin, make their appearance.

Differences are noticeable in the readiness with which autolysis of organs takes place under different pathological conditions. Jaeoby found that liver autolysis was much accelerated in animals poisoned with phosphorus; and I have made some tentative experiments upon the rapidity with which organs from infected and non-infected human beings undergo this change.

Many of you will have been impressed with the remarkable softness presented by the liver, spleen, kidneys and other organs in persons who have succumbed to typhoid fever, peritonitis, septicæmia, etc. This softness has nothing to do with putrefaction, and I think it reasonably certain that it is the result of autolytic processes which may have begun to operate even before death. The changes through which the acutely swollen spleen, such as is met with in typhoid fever, goes illustrate very well the manner of action of these ferments. The diffluent quality of the spleen in this disease quickly develops outside the body; and it can be shown to be independent of the post-mortem growth either of the typhoid bacilli or putrefactive bacilli. Every pathologist has seen a moderately firm spleen outside the body become soft and semifluid in a few hours; and I find that, reduced to pulp and placed under toluol-water, the same change takes place

In view of the resemblance of the hepatic lesions of advanced phosphorous poisoning to those of the liver in acute yellow atrophy, and the frequency with which the latter obscure disease supervenes upon acute infections, and, further, in view of the close agreement of the chemical products of degeneration of liver tissue in yellow atrophy with those of autolysis of the liver, the question arises whether the hepatic lesions in acute atrophy may not be the result of active autolytic processes set up by some agent that is as yet unknown.

The alterations in structure and consistence of muscle occurring in certain acute diseases (such as the so-called Zenker's degeneration which appears in typhoid fever) have features in common with changes noted in autolysis. The softening and preparation for absorption of infarcted areas in the brain, kidneys, spleen and other organs are also attributable to the action of intracellular ferments; and a similar form of softening occurs, if imperfectly, in malignant tumors, notably carcinoma, and in syphilis, where ferment action is possibly accelerated through the use of drugs, of which potassium iodide is the most efficient in use.

On the other hand, not all dead tissues are subject to this digestive liquefaction and absorption. Tuberculous foci, though degenerated, are remarkably persistent; and, as Prudden has shown, undergo softening probably only when certain bacteria invade secondarily the necrotic areas. It is of importance, in this connection, to recall that streptococci, which in themselves are not energetic tissue dissolvers, bring about softening and cavity formation in tuberculous foci; and the question as to whether their action is a direct one through products of their growth, or an indirect one by causing an increase in autolytic ferments, is pertinent but not as yet to be answered.

Fresh pus obtained, let us say, from an empyema, or fresh sputum, kept under toluol at the body temperature, becomes fluid, and a creamy layer collects upon the surface. The cells and nuclei disappear, and there appears in the fluid, as Naunyn first ascertained in 1865, various distintegration products, among which are leu-

cin, tyrosin, xanthin, guanidin, etc. Pus which thus undergoes digestion is capable of dissolving fibrin and even portions of organs independent of the action of bacteria. From this the conclusion can be drawn that pus, or really the leucocytes of pus, possess active digestive properties, a fact which the history of abscess formation and the removal by leucocytes of necrotic and other kinds of tissue, renders easily comprehensible. Many years ago Weigert attempted to explain, by assuming the operation of a peculiar poison which prevented fibrin-formation, the absence of fibrin from abscesses, etc., in which the fibrin factors must have originally been present. It is much more probable that what happens is a digestive transformation of fibrinogen, or of fibrin at the moment of its formation, by the ferments of the pus cell.

Broadly speaking, exudates and necrotic tissue are removed in two ways: (1) by absorption, and (2) by organization. In the first mode disintegration and solution of the cells, etc., with the exception of the fat and certain other elements, such as pigment, take place, and an emulsion results well adapted for entrance into the lymphatics. In the second, new vessels develop and invade the exudate or necrotic tissue, and by supplying a fresh set of leucocytes to dispose of the offending material, it is finally removed. From what has already been said, the first series of changes will readily be recognized as caused by autolysis; but the operation of the same cause is not so apparent in the second series. And yet, the two series are essentially the same. In the one the original material contains ferments of a kind and a quantity sufficient to bring about the transformation which is necessary before absorption can take place; in the second, the ferments being originally insufficient, are renewed by fresh leucocytes which emigrate from the vessels, load themselves with débris, and finally accomplish their entire removal.

There is little doubt that in many pathological conditions the leucocyte is the essential agent in bringing about absorption; and what is required to accomplish this end is not living leucocytes so much as large numbers of these cells, since autolysis proceeds independently of the vitality, as such, of the cells. The fate of pathological formations is dependent in large part on the numbers of leucocytes present within them.

The different behavior of a caseous and croupous pneumonia; the facility with which the one and the difficulty with which the other undergoes resolution is to be ascribed probably in part to the absence in large measure of leucocytes from the tuberculous process and their presence in enormous numbers in the acute inflammatory condition. Other examples illustrating the importance of leucocytes in promoting autolysis and absorption might be given.

I have been interested for the past two years in studying autolysis of the exudate in the lung in two inflammatory conditions, namely, acute lobar pneumonia and unresolved pneumonia. The pathology of the latter condition, except so far as the organization of the exudate is concerned, is, as you know, involved in the deepest obscurity. The study of the histology of the lung in various stages of the process of organization emphasizes one pathological condition, the import of which appears great in view of our present knowledge of autolysis; the exudate in unresolved pneumonia is fibrinous rather than cellular, and many of the alveoli of the lung are filled with dense hyaline fibrinous masses. All attempts to explain upon ordinary etiological grounds the peculiar changes, or absence of changes, in unresolved pneumonia, have failed; and

evidently the peculiarity of the process is to be sought in other causes.

Friedrich Müller first studied autolysis of the lung in croupous pneumonia, and described in detail its occurrence and the chemical products, among which are lysin, leucin, tyrosin, purin bases and phosphoric acid, of the digestive process. I have found that it is in the stage of gray hepatization that autolysis takes place quickly and perfectly, while in the stage of red hepatization it is very imperfect—a fact that can, I think, be attributed to the small number of pus cells present in the latter condition. But if the lung in unresolved pneumonia is exposed to conditions favoring autolysis, the process is slow and incomplete as compared with what takes place in gray hepatization. In gray hepatization, autolysis after death is a mark of the tendency during life of the exudate to become absorbed; in unresolved pneumonia the absence or reduction of autolysis is equally an indication of the future fate of the exudate, namely, during life to undergo organization.

I am, therefore, inclined to view unresolved pneumonia as an acute lobar pneumonia in which the inflammatory exudate, either because of some disproportion between the leucocytes and other constituents, or other cause as yet unknown, failing to autolyze perfectly, can not be absorbed, and hence undergoes organization.

SIMON FLEXNER.
UNIVERSITY OF PENNSYLVANIA.

## SCIENTIFIC BOOKS.

*A Revision of the Lepidopterous Family Sphingidæ.* By the Hon. WALTER ROTHSCHILD, Ph.D., and KARL JORDAN, M.A.L., Ph.D. Novitates Zoologicæ, Vol. IX., Supplement. Issued at the Zoological Museum, Tring, April, 1903. Pp. cxxxv + 972; plates I–LXVII. 4to.

This great work, based upon the splendid collections contained in the museum at Tring, and also upon all the other large collections in Europe as well as those in America, which have been carefully consulted, has occupied the learned authors fully eight years in its preparation. It is truly *opus magnificum*. On every page it gives evidence of the most painstaking and minute research, and is the first really satisfactory attempt to collate and bring into systematic review what has been done during the past one hundred and fifty years in relation to the large and interesting family of insects with which it deals.

The work falls into three parts: The Introduction, covering one hundred and thirty-five pages; the descriptive portion, occupying eight hundred and thirteen pages; and a Synonymic Catalogue of the Sphingidæ of the World, to which one hundred and sixty-seven pages are allotted. Sixteen of the plates are devoted to figuring hitherto little-known or hitherto undescribed species. These plates are executed in photo-colortype, or by the half-tone process. The remaining fifty-one plates, which are beautifully engraved upon stone, are devoted to the illustration of anatomical details. Evidently neither labor nor expense has been spared in making the treatise one of the most satisfactory pieces of monographic work which have ever issued from the press.

The introduction has value not merely for the lepidopterist, but for all students of the biologic sciences, inasmuch as the laws and methods of procedure, which should govern in systematic work, are taken up and discussed at length. The statements which are made as to the principles of nomenclature are especially worthy of study, and the conclusions reached are such as undoubtedly command the respect and win the adherence of all those who are sufficiently well versed in this subject to appreciate the position taken by the authors.

The hawkmoths are divided into two great groups, the Sphingidæ Asemanophoræ, including the subfamilies Acherontiinæ and Ambulicinæ; and the Sphingidæ Semanophoræ, including the subfamilies Sesiinæ, Philampelinæ, and Chœrocampinæ. The 'law of priority' has been strictly applied in ascertaining the generic names, which should be used.

The result may appear, to the student who is familiar with current nomenclature, in some cases strange, if not even startling, but the evidence submitted for the entire correctness of adopting the changes from current usage is, in the judgment of the present writer, cogent, and in almost every case entirely convincing. So far as the nomenclatorial adjustments touch familiar North American species, it may be worth while to point them out.

The species named *cingulata* by Drury is referred with its congeners to the genus *Herse* Oken. The genus *Protoparce* Burmeister receives into its embrace our species *sexta* = *carolina* Linnæus, *quinquemaculatus* = *celeus* Hübner, *occulta, rustica* and *brontes*. For the species named *hageni* Grote the genus *Isogramma* is erected; for *cupressi* Boisduval the genus *Isoparce* is proposed and described; and for *elsa* Strecker the genus *Dictyosoma* is set up. For *Sphinx plebeja* Fabricius the authors propose and describe the new genus *Atreus*. Inasmuch as *Atreus* is preoccupied in the Arachnida by Koch, the present writer proposes to substitute for it the generic name *Atreides* and this name will be given to the genus in 'The Moth Book,' which is now going through the press. To the genus *Hyloicus* are referred the species hitherto generally assigned to the genus *Sphinx* in American lists. Our species *modesta* Harris, which has recently quite erroneously been referred to the genus *Marumba* Moore, is put into the genus *Pachysphinx,* which is erected for its reception. Inasmuch as the type of the genus *Sphinx* Linnæus is undoubtedly *ocellata* Linnæus (see 'Systema Naturæ,' Ed. X., p. 489), the American congeners of this species are placed in that genus, and the name *Smerinthus* Latreille, hitherto almost universally applied to them, is dropped as a synonym. As the type of the genus *Sesia,* erected by Fabricius, is undoubtedly the species named *tantalus* by Linnæus, this generic name is retained for that species and its congeners. This will no doubt provoke protest from recent authors, but the step is logical, consistent, and in fact the only one which can be taken unless the 'law of priority' is to be set aside and disregarded. The generic name *Hæmorrhagia* is applied to *thysbe* Fabricius and its allies, while the genus *Macroglossum* Scopoli, of which the European *stellatarum* is the type, is placed in the Philampelinæ, at a wide remove from *Hæmorrhagia* (*Hemaris* auctorum), with which it has hitherto commonly been associated. Our common Morning Sphinx falls under the arrangement adopted into the genus *Celerio* and appears as *Celerio lineata.*

The work deserves the most careful study, and will remain a monument to the learning and the liberality of the distinguished nobleman and his erudite colleague, who have prepared it.

W. J. Holland.

Carnegie Museum,
June 12, 1903.

*Variation in Animals and Plants.* By H. M. Vernon, M.A., M.D., Fellow of Magdalen College, Oxford. New York, Henry Holt & Co. 1903. Pp. 415.

Since Darwin's 'Variation of Animals and Plants under Domestication' we have had no general résumé of the principles of variation. Yet this period has witnessed the rise (and fall) of many speculations on the subject, and for the past decade has yielded the solid fruits of biometric and experimental investigation.

This important gap is now filled by the well-arranged collection of data to be found in Vernon's book. These data are considered under three main headings as follows: 'The Facts of Variation'; 'The Causes of Variation'; and 'Variation in its Relation to Evolution.' In the first part some of the results of biometry are given without going into the more abstruse mathematical methods. In connection with the discussion of discontinuous variation De Vries's theory is considered in some detail. The causes of variation are classified as blastogenic and environmental, and several chapters are devoted to the latter class. In the third part the author discusses the action of natural selection on variations, and gives some of the evidence for the inheritance of acquired characters, based on the cumulative effects of the conditions of life

(where we miss the results of Standfuss and Fischer), and finally discusses adaptive variation. The author admits the importance of self-adaptations, which are, however, in his opinion, of little effect without natural selection. 'Degeneration' is a difficulty that the author does not attempt to compass, while admitting the unsatisfactory nature of Weismann's explanation. He should remember that the theory that phylogenetic 'degeneration' is due to disuse has inadequate support, and that animals with 'degenerate' organs, however produced, can still be adapted if they get into situations where such organs are of no use.

The book will be a welcome one to those who desire quickly to get at the recent literature on variation. The data are given in an impartial, sometimes even colorless way. The book lacks the vigor of the special plea and the enthusiasm of the book of one idea. It will be found very useful; but it will not found a school. C. B. DAVENPORT.

## SOCIETIES AND ACADEMIES.

### NEW YORK ACADEMY OF SCIENCES. SECTION OF GEOLOGY AND MINERALOGY.

A REGULAR meeting of the Section of Geology and Mineralogy was held at the rooms of the American Museum of Natural History on the evening of April 20, with Professor Kemp in the chair. Dr. A. A. Julien presented the results of his work on the hornblende schist which occurs at the extreme northern end of Manhattan Island near Spuyten Duyvil Creek. He was able, in the first place, to prove the undoubted igneous origin of this rock by the unaltered crystals pointing to an original gabbro which it still preserves. The speaker then presented his views in favor of the igneous origin of all the hornblende schists of Manhattan Island.

The second paper was by Mr. D. W. Johnson, on 'The Geology of the Cerrillos Hills, New Mexico.' The Cerrillos Hills form the most northerly group of a series of four laccolithic mountain masses in north-central New Mexico. The relation of these hills to the associated Cretaceous beds and the age of the intrusion were discussed. A brief petrographical description of the several igneous rocks was given and the subdivision and correlation of the sedimentaries on paleontological grounds considered. The origin of the anthracite coal of the Madrid area and the origin of the famous turquoise deposits of the hills were then treated. The speaker closed with a résumé of the geologic history of the region. Professor Kemp led in the discussion which followed. Dr. H. S. Washington was asked by the chairman to calculate an analysis of the type of andesite which is found in the Cerrillos Hills.

GEORGE I. FINLAY,
*Secretary pro tem.*

ON May 18 the first paper was by Dr. George I. Finlay, of Columbia University, and was upon 'The Geology of the Nephelite Syenite Area at San José, Tamaulipas, Mexico.'

In this paper Dr. Finlay said in part: The town of San José in the state of Tamaulipas, Mexico, lies in a hollow surrounded on all sides by mountains, and is about seventy miles from the coast of the Gulf of Mexico. The range of peaks immediately to the south of it, and extending for fifteen miles in that direction, is of nephelite syenite. The range is known as the San Carlos Mountains. San José itself is on the site of an eroded laccolith of andesite (locally known as 'porphyry'), intruded into limestone. Some limestone masses stand on end within the areas of the laccolith, and are thought to have floated or worked their way down to their present position during the intrusion of the igneous rock. There are two or three hundred of these isolated limestone masses, and it is in connection with these that the copper ores are found. Contact metamorphism has not been developed to any great extent in the limestone surrounding the laccolith, but has been greatly induced in the included masses, marble, grossularite, vesuvianite and several other minerals being the products. Aside from the occurrence of the nephelite-syenite in the area south of the laccolith, the region is interesting on account of the dyke rocks which are found cutting the andesite of the laccolith. Among these are analcite-

tinguaites and camptonites, as well as vogesite and diabase. Two main streams now drain the hollow formed by the down-cutting of the dome where the weaker andesite has been laid bare as far as the limestone cover has been cut back.

Dr. Finlay's paper was discussed by Professor Kemp, who called attention to the fact that the character of the intruded limestone was not yet entirely clear; and by Dr. H. S. Washington, who dwelt on the interest attaching to the additional localities here and elsewhere recently reported for the peculiar dyke rocks mentioned.

The second paper of the evening was by Fred H. Moffet, Columbia University, and was entitled 'The Copper Mines of Cobre, Santiago de Cuba.'

In this paper Mr. Moffet said in abstract: The copper mines of El Cobre are located about nine miles west of the Bay of Santiago, where a series of eruptive flows, andesites and rhyolites, are interbedded with fragmental rocks, agglomerates, breccias and tuffs. The strike of the beds is east and west, and they dip at a low angle to the north. The series is cut by trap dykes and by two major systems of faults, the older of which runs east and west and carries with it the large ore bodies. The second major system has direction nearly north and south. Cross faults cut and displace the ore bodies of the older system, and carry copper, though in less amount. The copper workings of the old English mining companies produced enormous quantities of very rich oxidized ore which gave place in the lower levels to sulphides. Much difficulty is encountered in handling the mine water on account of the porous nature of the country rock. At the present time the iron ore of the region is of much greater commercial importance than the copper.

In the discussion which followed, Professor Kemp spoke of the great importance to the United States steel furnaces which these deposits possessed on account of their great extent and convenient location. The ore is extremely low in phosphorus but contains some sulphur. The copper may again be of great importance, though but little is being done at present toward its exploitation.

E. O. Hovey,
*Secretary.*

ANTHROPOLOGICAL SOCIETY OF WASHINGTON.

The 347th meeting was held May 12. Dr. J. Walter Fewkes, who recently returned from the West Indies, gave a brief account of his work in Porto Rico and Santo Domingo, reserving a fuller presentation till next meeting. The paper of the evening was by Dr. E. A. Spitzka, entitled 'Cerebral Characteristics of Distinguished Men, with special reference to the late Major J. W. Powell.' Following is an abstract:

Dr. Spitzka reviewed previous records of brains of distinguished men and proceeded to a consideration of their chief characteristics, particularly the brain-weight and the surface morphology—both generally and with special reference to certain cortical areas. Dr. Spitzka had tabulated the brain-weights of ninety-seven men eminent in the professions, arts and sciences; compared with the series of 'ordinary' brain-weights collected by Bischoff and Marchand, there was a relatively and decidedly greater number of heavier brains in the former series. In a further analysis it was shown that the brains of men devoted to the higher intellectual occupations, such as the mathematical sciences, involving the most complex mechanisms of the mind, those of men who devised original lines of research (Cuvier, Cope, Agassiz) and those of forceful characters like Ben Butler or Daniel Webster possess the heaviest brains. The increase in brain-weight during evolutionary progress is directly related to the increase of cranial capacity along with the development of brachycephaly. As for the cerebral surface appearances, the highly intellectual man is likely to exhibit not only a superior degree of fissural and gyral complexity in general, but of certain cortical fields in particular. These differences in the extent of certain cortical (especially the associative) areas are palpable and measurable. Particularly important in this relation is the insula (Island of Reil), probably the purest

association center in the brain, and usually showing a superior degree of development in men whose powers of speech were of a high order.

The brain of Major J. W. Powell (to be described in the *American Anthropologist*) exhibited a decidedly superior degree of development. Its weight, 1,488 grams, was above the average of the 97 brains of eminent men, and for a man below medium stature and of rather small frame and whose age was 68, it was notably above the average. The most notable feature is a redundancy in the subparietal association area (especially the angular gyre) on the right side, a feature which may not be unrelated to an important characteristic of Major Powell's mental make-up: that of keen observation and superior powers of generalizing these. A large number of charts and figures of brains of notable persons illustrated the paper.

In discussing the paper Dr. Frank Baker held that the convolutionary pattern of the brain is due to the needs of motor activity, and any conclusions from it as to brain power should be carefully weighed.

Professor W J McGee stated that Major Powell's strength lay in generalizing on observations in nature and that the address of Dr. Spizka shows that it is now possible to trace a definite relation between cerebral structure and the psychical character of an individual. Dr. Ales Hrdlicka said that it is a well-known fact that every organ in the body, if more than ordinarily exercised, receives an augmented blood-supply and in consequence tends in time to increase in size and weight. It would be very strange if the brain formed an exception to this law. It is true that the efficiency of a brain may increase by the advance of the differentiation of its minute elements, but in all probability this and actual growth go hand in hand, and the size and weight of the brain must be of considerable importance in the study of the organ. That no very definite results in this respect have as yet been generally arrived at is due to the fact that we are practically only in the beginnings of brain study and need many additional accurate data on normal material, and that not only in white but also in various more primitive peoples. We need many further data on the significance in brain study of race, sex, stature, muscularity, age as well as other factors. The significance of the convolutionary pattern is particularly in need of further investigation, yet it is a general opinion that a marked complexity of the convolutions goes with the superior brain and *vice versa*. Dr. Hrdlicka expressed a hope of establishing in the course of time a valuable brain collection in his division in the U. S. National Museum.

Further remarks germane to the topic were made by Dr. D. S. Lamb, Dr. J. Walter Fewkes, Mr. G. K. Gilbert and Mrs. Miranda B. Tulloch. Dr. Spitzka made some closing remarks. At the conclusion of the meeting a vote of thanks was tendered Dr. Spitzka. It is expected that the paper will be published in a forthcoming number of the *American Anthropologist*.

WALTER HOUGH,
*General Secretary.*

## THE BOTANICAL SOCIETY OF WASHINGTON.

THE fifteenth regular meeting of the society was held at the Portner Hotel, May 23, 1903, with President A. F. Woods in the chair and twenty-four persons present. At the close of the business meeting, Dr. W. H. Evans, chairman of the scientific program for the evening, was called to the chair.

Dr. R. E. B. McKenney communicated 'Notes on *Saccharomyces niger*.' This rather uncommon fungus, which has been considered to be a true yeast, can by certain methods of culture be made to produce a well-developed, branched mycelium. Under such conditions the mycelium is septate, while under others it is unseptate. True ascospores were not observed; and it was considered probable that Marpmann, who claims to have found them, mistook certain refractive metabolic bodies for such spores. The formation of false zygospore structures was also noted. The fungus is capable of continued growth for a couple of months in nutrient media, which is apparently free from nitrogen. It would

seem, therefore, that *Saccharomyces niger* is to be added to the small list of fungi which are thus capable of assimilating free nitrogen from the air. Mention was also made of certain other of the nutrition phenomena of this fungus.

Dr. George T. Moore gave a very interesting address on a new method of artificially inoculating soils for legumes with the nitrogen-assimilating, tubercle-forming bacteria. The practical application of the method is very simple and was fully described. The paper will be published in full as a bulletin of the Bureau of Plant Industry, U. S. Department of Agriculture. H. J. Webber.

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## *Discussion and Correspondence.*

### The Grand Gulf Formation.

In Science of December 12, 1902, Professor Dall, in commenting on our note on the Grand Gulf Formation, published in the number for November 21, 1902, calls our attention to two errors, which we now acknowledge and are very glad to correct. We made the statement that Dr. Hilgard had considered the Grand Gulf as of Eocene age. This is a mistake which escaped us both in the manuscript and in the proof-reading. Since Dr. Hilgard's work forms the basis of all our knowledge of the Gulf Coastal Plain, we knew from long-continued study thereof that there was not a line in all his writings which could be interpreted as even suggesting this age for the Grand Gulf. So also we were mistaken in saying that Professor Dall had regarded it as of Eocene age.

It is, furthermore, evident from Professor Dall's criticisms that we have not stated our case with sufficient clearness to prevent misunderstanding of our position. Inasmuch as to us the facts in our possession seem to afford absolute proof of the correctness of our conclusions, we beg to submit the evidence somewhat more fully to the consideration of the geologists interested.

At the outset it seems necessary to define clearly what we mean by Grand Gulf, and we can do no better than to follow Hilgard, who has so well described these beds, and who has correctly mapped them as covering the lower part of the state of Mississippi from the southern limit of the Vicksburg down to within a few miles of the Gulf of Mexico.

The materials of the formation are sandstones, sands and clays, with silicified trunks of trees and beds of lignite, and lignitic clays containing leaf impressions, badly preserved and incapable of determination. Concerning these Grand Gulf beds Dr. Hilgard remarks: "Two points confront us in the discussion of the relations of the formation to the sea; the great rarity of the calcareous feature in the main body of the formation, and the utterly 'unmarine' character of the materials generally, in the constant recurrence of the lignito-gypseous facies." And again, "Of the sweep of 900 miles thus outlined as the known extent of this formation, 400 may be considered as having been examined sufficiently in detail to prove the absence of marine fossils from the formation; the portion so examined embracing, moreover, its widest part and fully two thirds of the area of the outcrop."*

By the characters thus outlined, this great fresh-water formation has been recognized and described by the geologists in Georgia, Florida, Alabama, Mississippi, Louisiana and Texas. No one has had any serious difficulty in distinguishing it in the field; but every one has had difficulty in reconciling the known facts of its surface distribution with any satisfactory assignment of it to a definite place in the stratigraphic column. The only formation with which it is at all likely to be confounded is the Lafayette, which everywhere, according to Hilgard and other geologists, directly overlies it, and of which the materials are often quite similar; and we have conclusive evidence that parts of the Grand Gulf have by several authors been included in the Lafayette.

In their relations also to the underlying older strata, these two formations have much in common; for instance, they both 'blanket' a number of older formations, but the Grand Gulf, so far as yet known, overlaps only Miocene, Oligocene and Eocene as far down as

* *Am. Jour. Sci.*, Vol. XXII., July, 1881.

the Tallahatta Buhrstone, or lower Claiborne.

While, therefore, we appear to be in substantial agreement as to the *characters* of the formation which we call Grand Gulf, we differ radically from Professor Dall as to the *place* which it occupies in the stratigraphic column.

We fully concur in his statement that, "It can not be too often emphasized that no determination of the age of its (southern coastal plain) beds not based on their fauna, or the fauna of beds both above and below those in question, can be regarded as more than tentative; and such determinations in the past have almost invariably proved erroneous."

The sole purpose of our first note was to prove, by the application of these very principles, that the stratigraphic position of the Grand Gulf beds was between the Pascagoula Tertiary and the Lafayette; but since doubt still remains, we wish to offer a few additional considerations.

So far as we have been able to ascertain, the Grand Gulf beds *themselves* do not anywhere contain the fossils which afford incontestable evidence of their age. Dr. Hilgard writes:* "Apart from this [the finding of a few fragments of a turtle shell], my most patient search, in hundreds of localities, has failed to produce any fossil form; even the leaves associated with the lignite seams being so ill-preserved as to be unrecognizable."

And though casts of fresh-water shells have since been found in the formation, no determination of its age from these has been possible,† and we are thus compelled to rely wholly upon the other test, viz., the fauna of the beds below and above the ones in question.

If we consider *first* the formations which are known to *overlie* the Grand Gulf, there is not very much to be said, but it is conclusive.

The case in Texas is thus given by Professor Hill, in a recent letter: 'The so-called Grand Gulf beds of the Texas region *are not overlaid by the Tertiary.*'

In Mississippi we have Hilgard's testimony, as follows: "The latter (stratified drift or Lafayette) is found directly capping almost everywhere, the claystones and sandstones that characterize the highest part of the Grand Gulf group."

In Alabama also the Lafayette is nearly everywhere seen capping the Grand Gulf, and we have no record of anything older than Lafayette in this relation to it. The same thing is certainly true with regard to western Florida, and, we have no doubt, to the rest of Florida and Georgia as well.

In Bulletin 84 of the U. S. Geological Survey, Professor Dall says: "There is no doubt that directly in contact with the Grand Gulf beds in the Gulf states, lies the formation variously recognized under the names of Lafayette or Orange Sand of Hilgard, Lagrange of Safford, or Appomattox of McGee."

So while there are localities by the tens of thousands, in the Gulf states, where the Grand Gulf is directly overlaid by the Lafayette, we have no recorded instance of its being overlaid by any formation older than the Lafayette.

This circumstance alone affords at least presumptive evidence that the true place of the Grand Gulf is high up in the geological scale, and close under the Lafayette.

*Secondly,* as to the *underlying* formations. In Mississippi Dr. Hilgard found no contact of the Grand Gulf with any underlying formation other than the Vicksburg limestone. In connection with his description of these

* *Loc. cit.*, p. 59.

† Kennedy finds *V. planicosta* in Fayette sandstones, but in basal layers which Veatch considers Jackson; Veatch also, in Frio clays near Binkville, La., finds a fossiliferous (casts) layer in a ferruginous rock; Harris finds *Unio* and *Anodonta* casts at Chalk Hills, La., along with leaves of birch, willow and other dicotyledonous trees; Meyer has mentioned casts of fresh-water shells occurring also at Grand Gulf.

It may easily be imagined that the waters which were active in transporting and depositing the materials of the Grand Gulf might on occasion carry into it fossils of an older formation over which they swept. The finding of a few Eocene Miocene fossils in the Grand Gulf beds should not cause any more surprise than the finding of Sub-Carboniferous fossils, for instance, in the Lafayette, as has often been done.

contacts he points out a very significant fact, viz., "While the Vicksburg rocks show at all long exposures a distinct southward dip of some three to five degrees, the position of the Grand Gulf rocks can rarely be shown to be otherwise than nearly or quite horizontal, on the average; although in many cases faults or subsidences have caused them to dip sometimes quite steeply, in almost any direction."*

And generally in the Gulf states, the landward margin of the Grand Gulf almost invariably rests upon the Vicksburg limestone, and many sections have been published showing this contact. On the principle that, in the absence of evidence to the contrary, a formation follows next in chronological order, that formation upon which it directly rests, the Grand Gulf (or part of it) has usually been placed in the geological column, next above the Vicksburg limestone, *i. e.*, in the Miocene (or, as some now prefer to call it, Oligocene).

The application of this principle in Alabama would cause us to place the Grand Gulf at a number of horizons where we are perfectly certain that it does not belong, for while in most cases it rests upon the Vicksburg limestone, yet we have recently seen it in direct contact with the *Upper Claiborne* and upon the *Lower Claiborne* or *Buhrstone*, on the one hand, and upon the *Chattahoochee Miocene*, and directly upon, as well as far above, the *Pascagoula* (Miocene or Pliocene).

So far as we know, no one has ever placed the Grand Gulf between the lower and upper Claiborne, or between the latter and the Vicksburg. Too many sections have been described showing the contact of these formations without any intercalated Grand Gulf, to permit any such assumption.

But the relations of the Grand Gulf to the Vicksburg and post-Vicksburg Tertiary formations have, perhaps, not heretofore been fully set forth. Conclusive evidence as to these relations is afforded, we think, at the following localities:

1. *Chattahooche and Appalachicola River.*—If any part of the Grand Gulf occupies the position assigned by Professor Dall to his 'typical Grand Gulf,' *i. e.*, between the Vicksburg and the Chattahoochee limetones (or approximately at that horizon), there should be somewhere on the gulf coastal plain a section which would exhibit these beds in that relation to each other. So far as we are aware, no such section has ever yet come under notice.

Certainly we should expect to find such an exposure in that most complete and unbroken section of the later marine Tertiaries afforded by the Chattahoochee and Appalachicola Rivers. This series of the Neozoic rocks, discovered by Langdon in 1887, has been studied by a number of eminent geologists, including Professors Pumpelly and Gilbert Harris, and Messrs. Dall and Stanley-Brown, some of whom have published descriptions.

The most complete and carefully prepared account of this section is that which appears in Volume 5 of the *Bulletin of the Geological Society of America*, 'Cenozoic Geology along the Appalachicola River,' by Messrs. Dall and Stanley-Brown.

From this article we make the following quotations: "At a place on the left bank of Flint River a few miles above the Florida boundary line, known as Willey's Landing, Professor Pumpelly states that the contact between the Vicksburg and undisturbed Chattahoochee Miocene may be observed."

"Beginning at the base of the column, Professor Pumpelly has shown that the Chattahoochee series rests on an erosion surface of the Vicksburg or Orbitoidal limestone which forms the culmination of the Eocene. We have confirmed this by an examination of the fossils submitted by Professor Pumpelly."

In the '18th Annual Report' of the Director of the U. S. Geological Survey, page 330, Professor Dall says: 'There is no marked break in the stratigraphy between the Upper and Lower beds so far as yet observed.' The lower beds here referred to are the Vicksburg

* Many subsequent observations in Mississippi and Alabama confirm this, and show besides that the Grand Gulf beds, having been deposited upon an eroded surface of the Tertiaries, exhibit great variations in both the dip and the thickness.

and Ocala formations, and the upper are the Chattahoochee and Chipola.

Since no mention is made in these sections of any Grand Gulf, we may safely assume that none exists there between the Vicksburg and the Chattahoochee, and yet on the uplands, on both sides of the Chattahoochee River the characteristic Grand Gulf beds may be seen occupying the surface with the usual capping of Lafayette sands.

Mr. Gilbert Harris also publishes a good account of this river section in his 'Bulletin No. 15 of American Paleontology.'

In this article, as well as in that of Messrs. Dall and Brown, it is demonstrated beyond all question that the Chipola overlies the Chattahoochee, apparently conformably and certainly without the intercalation between them of any Grand Gulf beds; and in the same way the Chipola is conformably overlain by the Alum Bluff beds, and these in turn by the Chesapeake marl, followed by what Professor Dall calls an *aluminous clay,* which he considers as belonging probably also to the Chesapeake. "It will be seen on examination" (of the sections and diagrams) "that, while the series is not complete in any single section, taken collectively there is no gap outstanding between the beds and, humanly speaking, no room for misapprehension as to their position and age."*

Let us now trace up the Grand Gulf beds along these rivers. Nobody will deny that they overlie the Vicksburg limestone, both in Alabama and in Georgia, clear up to the Chattahoochee River where the post-Vicksburg series of marine Tertiary beds begins.

Mr. Harris† gives a section of the bluff at the old Chattahoochee Landing, in which, underneath the orange and red sands, presumably Lafayette, at the top of the section, there are some twenty feet of purplish clayey sands, and light sands and clays, which he says 'resemble Grand Gulf.' No doubt they *are* Grand Gulf, and here they overlie the Tertiary beds consisting, according to Professor Dall's determinations, of Chattahoochee and Chipola.

* Dall and Brown, *loc. cit.*, p. 162.
† *Loc. cit.*, p. 52.

Near the top of every section of Professors Dall and Brown there is shown a bed, their No. 2, which has been referred by them to the Lafayette, but which, from the descriptions, appears to include both the Lafayette and the Grand Gulf. For the authors emphasize the facts that these beds are nearly seventy feet thick, and that they are often different in composition, structure and color from the more homogeneous (Lafayette) formation to the northward.

It is safe to say then that in this Appalachicola and Chattahoochee section, from the Vicksburg limestone up to the top of the Chesapeake Miocene as shown at Alum Bluff, none of the beds which take part in the formation of the river bluffs has ever been considered Grand Gulf, unless it be those capping the bluffs, above all the Tertiaries, and covered only by the Lafayette and more recent deposits.

2. *Conecuh River, Escambia County, Ala.*—In the upper part of township 2, range 12, in this county, the bank of Conecuh* River is formed by the Vicksburg limestone, while in the lower part of the same township, some four miles distant, the bank is formed by gray sandy clays holding Miocene fossils. At both localities the Grand Gulf beds, sands, clays and lignites, overlie the Tertiary formations, and form the surface of the country intervening, with the usual capping of Lafayette.

3. *Chickasawhay River, in Greene County, Miss.*—About five miles above the confluence of Leaf and Chickasawhay Rivers on the latter stream, we have recently examined a bluff at the base of which is a shell marl, with innumerable shells of *Rangia Johnsoni,* and a few other forms characteristic of the Pascagoula horizon. Above this and forming the upper half of the bluff are typical Grand Gulf strata, sands and clays with lignite bed and silicified trunks of trees all *in direct contact* with the Pascagoula marl.

4. *Mobile County Artesian Borings.*—These borings have been made at and near Mobile and at Alabama Port. The deepest is the

* By inadvertence this was called Escambia River in our previous note.

Bascomb well near Mobile, to which we have referred in our first note. At this well the Grand Gulf, with its usual capping of Lafayette, forms the surface. The boring reaches the Pascagoula shell bed at about 700 feet, and the bed containing Oak Grove fossils (Chattahoochee) at about 1,500 feet. From the materials brought up we judge that about 180 feet at the top of this boring are in the Grand Gulf; how much, if any, more of the strata belongs to this formation we can not, of course, say, but the main point to be noted is that here the Grand Gulf lies *far above* the Pascagoula, and the latter some seven or eight hundred feet above the Oak Grove beds.

5. *The Coasts in Mobile and Baldwin Counties, Alabama, and in West Florida.*—We have already spoken in our first note of the occurrence of the Grand Gulf down to within a mile or two of the Gulf in Mobile County; of its occurrence in Baldwin County on the shores of Perdido Bay, where it makes a high bluff. Recently we have seen the same formation making the greater part of a bluff, thirty feet or more in height, on the very border of Pensacola Bay in the city of that name.

Now since a formation is bound to be *younger* than the newest formation which it overlies, and *older* than any which overlies it, we are forced by the facts adduced above to conclude that the Grand Gulf occupies a place in the geological column somewhere *between* the uppermost of the Tertiary formations as yet determined by its fossils, viz., the Pascagoula and the Lafayette. We will not even say that the Grand Gulf represents *all* the time and space between these two formations, for the borings near Mobile and the bluffs on Perdido Bay at Pensacola, show that if there are any post-Pascagoula Tertiaries in Alabama and western Florida, the Grand Gulf comes in between *them* and the Lafayette.

If any one should still maintain that the Grand Gulf belongs anywhere in the Tertiary column between the Buhrstone and the top of the Pascagoula, we think the burden of proof rests with him; he should show a single section where Grand Gulf beds, of the character described by Hilgard and accepted by all the geologists of the gulf coastal plain, may be seen intercalated between *any two* of the Tertiary formations.

If we seek to escape the legitimate conclusion from the facts above given of their distribution and stratigraphical relations, by assuming that the *fresh-water* Grand Gulf beds of the west (Mississippi and Alabama) find their *marine equivalents* further east in the Chattahoochee series, we are confronted with this fact, that at the Chattahooche River, and beyond through Georgia, the characteristic fresh-water Grand Gulf beds, such as Hilgard has described them, overspread the country just as they do to the west, showing no signs whatever of any transition into marine deposits. It is needless to seek equivalents when the thing itself is there.

And, moreover, we now know for a certainty that all across Alabama and in the type locality of Dr. Hilgard, on Chickasawhay River in Mississippi, both the *fresh-water Grand Gulf* and the *marine fossiliferous Tertiaries coexist* everywhere, the Grand Gulf above, the Tertiaries below.

The facts which we have presented above may easily be verified by a few days' field work. If they are susceptible of other construction than that which we have placed upon them, or if there are other facts incompatible with the conclusions which we have reached, we stand ready to modify or abandon our views, since we are fully aware of our limitations, and of the difficulty, almost the impossibility, of arriving at the *whole truth*. If we have made a small contribution towards it we ought to be satisfied.

No one can appreciate more than we do the great work which Professor Dall has accomplished in our southern Tertiaries, and we hope he may long continue active in the same work, and bring us finally to a certain and complete knowledge of the sequence of these formations and of their contained fossils. If we differ from him on some of the points presented above, it is because the facts seem to compel us thereto.

Moreover, we think he has hardly given due

weight to the evidence which we brought forward in our first note, and certainly he does not correctly represent our position in some of his comments on that note, as, for instance:

1. He thinks that our assumption of the late, possibly Pliocene, age of the Grand Gulf beds, if it should prove correct, is merely an equivalent of the idea of Dr. Hilgard cited by him, and, therefore, not new.

Dr. Hilgard's idea was that these beds represent *all* the time and space between the Vicksburg and the Lafayette; ours is that they represent *a very small part only* of that time and space, viz., the part between the Lafayette and the uppermost of the known Tertiaries of the Gulf coast—the Pascagoula, and very probably not even all of that. If a part is equal to the whole, then the equivalence is established, and we stand convicted.

2. "The beds which Messrs. Smith and Aldrich call 'Grand Gulf' in their communication to SCIENCE are not the same" (*i. e.*, Professor Dall's 'typical Grand Gulf') "but are the non-fossiliferous upper portion at the other end of Hilgard's Grand Gulf section."

The clays containing lignite and fossil palm leaves, described by Hilgard at Powe's on Chickasawhay River in Mississippi, have been fixed upon by Professor Dall as his 'typical Grand Gulf' beds, and have been classed by him as Oligocene (SCIENCE, December 12, 1902, p. 946). Now from this type locality Dr. Hilgard traveled southward down Chickasawhay and Pascagoula, giving details of exposures of the Grand Gulf as far as Dwyer's ferry, ten or twelve miles from the gulf. Although he then saw no contact of the Grand Gulf with any underlying formation, owing, no doubt, to the stage of the water, yet this formation may be seen, as above described, resting on the Pascagoula shell marl on the Chickasawhay, a few miles above its confluence with Leaf River. And furthermore, the Grand Gulf at this point consists of clays, sands and lignitic clays, with silicified trunks of trees. The same formation is at the surface thence back to the type locality near which it is seen resting upon Vicksburg limestone. Here then the unquestioned Grand Gulf, with Hilgard's hall-mark of genuineness upon it, rests upon the beds of Upper Miocene (or Pliocene) age, called the Pascagoula, just as they rest upon the Eocene Vicksburg limestone twenty miles further north. Both Hilgard and Johnson have given details of numerous other fossiliferous Grand Gulf beds in these parts of Mississippi. These are the beds which we have called Grand Gulf. And furthermore, we find lignites and lignitic clays with leaf impressions in the formation overlying the Miocene at Coal Bluff and Roberts in Escambia County, Alabama; we find the lignitic matters in the same beds down to the water's edge on Mobile Bay, so that certainly all the beds which we have been calling Grand Gulf *are fossiliferous* precisely as are those described by Hilgard which Professor Dall accepts as his typical Grand Gulf. Nor have we been writing about the upper part of the formation only, as the bluffs on Conecuh River demonstrate. If the Grand Gulf beds which there rest on the Vicksburg limestone be counted as the lower beds, then the beds of the same formation resting on Miocene fossiliferous sands, four miles further south, can not be very much higher up, especially in view of the fact that the Grand Gulf beds are everywhere very nearly horizontal. And even upon the assumption of a steeper dip, since the Grand Gulf covers the country down to the gulf, a distance of thirty or forty miles from the Conecuh River localities, those beds which form the surface over only four miles (or one tenth of this area) certainly would not belong to the upper portion.

3. "By means of paleontological data, * * * I have been enabled to fix the age of *different portions* of the original heterogeneous series as uppermost Oligocene (transitional) and Chesapeake Miocene, which is fully confirmed by the facts now cited by your correspondents."

Langdon's discovery along the Chattahoochee in 1887 first showed beyond all question that fossiliferous marine beds occupied a part of the geological column which Hilgard once thought to be covered by his Grand Gulf beds alone. Langdon gave the name Chattahoochee to these deposits, which he

classed as Miocene, and this determination of the age has been very generally accepted by the geologists. Professor Dall's critical studies of the fossils and stratigraphy of this region have since enabled him to give greater exactness and precision to Langdon's first outline by establishing several substages of the original Chattahoochee; by assigning the lower part of the original Miocene to the Oligocene, and the upper to the Chesapeake Miocene, etc.

Now, while the facts given by us certainly confirm the existence of Langdon's Chattahoochee beds, by whatever name now known, below the Grand Gulf in western Florida, and in Alabama and Mississippi, we do not see how they either confirm or contradict the conclusions of Professor Dall as to the true age of the different parts of the Chattahoochee series.

To summarize:

1. The Grand Gulf of 'Messrs. Smith and Aldrich' is the same fossiliferous formation which Hilgard has described by that name, and not merely 'the upper non-fossiliferous portion at the other end of Hilgard's section.' It is the same formation which Professor Dall calls the 'typical Grand Gulf' in his recent communication, and which he considers Oligocene, and a remnant of the heterogeneous Grand Gulf of Hilgard. We are compelled by the facts to believe that this typical Grand Gulf is not Oligocene at all, but that it belongs about a quarter of a mile vertically above the place in the geological scale to which it is assigned by Professor Dall.

2. There is also no Miocene Grand Gulf, as Langdon's discovery has proved and as has been confirmed by other geologists who have studied the Chattahoochee-Appalachicola section. We might perhaps more correctly say there is no Miocene Grand Gulf below the horizon of the Pascagoula, if that be certainly proved to be Miocene.

3. We think our facts prove that the Grand Gulf, all and singular, occupies a place in the geological column *below* the Lafayette and *above* the Pascagoula (which is the uppermost of the Tertiary formations as yet determined along the Gulf coast). This is all we have endeavored to show, and it was the *raison d'être* of our first note. We do not see wherein what we have there said in any way confirms Professor Dall's 'earlier determinations,' and, furthermore, we think that our view of the age of the Grand Gulf is new, and not a mere equivalent of the views of any other geologist.

Eugene A. Smith,
Truman H. Aldrich.

## *SHORTER ARTICLES.*

### THE REMAINS OF BEAR AND DEER ON THE SHORES OF ONONDAGA LAKE.

Through the courtesy of the firm of Will & Bauer, of Syracuse, the university received in March parts of the skulls of six bears (*Ursus americanus*); the leg bones of at least three bears (*Ursus Americanus*); and parts of the leg bones of three deer, the Virginia deer. The bones were identified through the kindness of Dr. Ablen, of the Natural History Museum of New York.

The find was made north of Syracuse, about one eighth of a mile east of Onondaga Lake, while the company was excavating for a trench. The land at this place is level, the surface soil is of a mucky character, varying in thickness from three to nine feet. Beneath the muck there is a bed of marl with here and there some quicksand.

The workmen noticed bones when they first began digging, but failed to bring the fact to the attention of the foreman until the work was nearly completed. A careful watch was maintained during the remainder of the excavations, with the above gratifying results. The bones were not taken from one place, but were found scattered over an area of several square rods, a skull being found at one place and the jaws at another.

A brief description of the bones may be of some general interest. The larger skull measured twelve inches from the occipital ridge to the premaxilla; nine and one half inches from the anterior side of the foramen magnum to the premaxilla; three and one half inches from the right zygomatic arch to the sagittal plane of the skull; the left

zygomatic arch was destroyed. Large portions of the occipital and parietal bones were missing, as if the skull had been crushed in killing; there was no evidence of recent change. The smaller skull was the more complete. The distance from occipital ridge to premaxilla was ten and one half inches; from the foramen magnum to the premaxilla, six and one quarter inches; the zygomatic arches were both broken and there was a hole in the occipital and left parietal, as if the animal had been shot. The complete mandible belonging to this skull was found.

In addition to these two skulls, which are of the black bear, there were parts of four different mandibles of the same variety, one of which was a full inch longer than the mandible of the larger skull. The incisors are present in this large mandible, and a number of rudimentary premolars were found in several of the mandibles.

The leg bones of the bears consist of the following: one pair of humeri ten inches long, incomplete, the proximal ends being absent. These two humeri are so similar that I believe that they belonged to the same animal. One right humerus nine inches long, having the proximal ends present; the distal ends of two left humeri; a left tibia and fibia which are united, and a right tibia that is so similar to the left one that they undoubtedly belonged to the same animal. Judging from the number of humeri, we have at least the remains of three bears and possibly a fourth.

The deer bones are one incomplete humerus and radius, eight and one half and nine and one half inches, respectively, in length, and a complete ulna eleven and five eighths inches long. These three bones articulate perfectly. One tarsal nine inches long, that articulates with the radius and ulna. A second set of leg bones that articulate also, indicating that they are from the same individual. One humerus eight and one half inches long, the proximal end absent; one radius nine and three fourths inches long, complete; one ulna five and one half inches long, incomplete; two broken humeri; three miscellaneous vertebræ; six ribs, and the part of an antler. This would give us the remains of at least two deer.

All these bones are recent and do not show any evidence of mineralization. The two skulls and the mandibles and leg bones of the deer still show evidence of animal oil, being slightly oily to the touch. The leg bones of the bears are drier than the others and the epiphysial joints are plainly evident when present, but in most of them the articular surface is lost.

It is difficult, with the few facts at our command, to estimate the age of these bones. I am inclined to think that they are not very old, possibly a hundred years. Until we possess more bones and know more of the geology of the place, any statement concerning their age must be mere conjecture.

As to the manner in which the bones reached this place, a few suggestions may be made. Onondaga Lake is of glacial origin, like most of the central New York lakes. At each side and south of it, the old valley is filled with glacial débris to a depth of several hundred feet, the present lake beach being some distance from the rock strata that limits the valley. Along the hillsides of the region about Syracuse there is evidence of beaches, showing the limits of the lake in prehistoric times. The place where the bones were exhumed probably represents a part of the lake previous to its last subsidence. From the scattered condition of the bones it is easy to imagine how they may have been washed down from the banks, being possibly the remains of an Indian feast.

The present collection is of sufficient interest to warrant some care in future excavations in the above area.

W. M. SMALLWOOD.
SYRACUSE UNIVERSITY,
ZOOLOGICAL LABORATORY,
May 1, 1903.

## *BOTANICAL NOTES.*

### THE STUDY OF WOOD.

WITH the rapid increase in interest in all matters pertaining to forestry, so notable in the past few years, there has been a corresponding increase in the number of books devoted to some phase of the subject. The

latest contribution is a volume of about two hundred pages entitled 'The Principal Species of Wood; Their Characteristic Properties,' by Professor Charles H. Snow, of the School of Applied Science of New York University, and published by John Wiley and Sons. In his preface the author says of the book that it is 'a brief, untechnical presentation of general features characterizing economically important species of wood.' This should be understood as implying that the presentation is untechnical from the botanical standpoint only, for it is emphatically a technical book, in so far as it is designed for the use of foresters, engineers, builders and dealers in wood (lumber) of all kinds. It is written for and appeals to men of these classes, and to that extent it is a technical book.

The book opens with an introductory chapter containing such explanations as will make the text more readily understood. Then follow thirty-six sections, each devoted to a group of similar woods, and here each section opens with an introductory statement in regard to the species discussed in it. The treatment of the particular species may be illustrated by that of the white oak, which covers the following topics: Nomenclature (including English and Latin names, as well as the more common synonyms); locality; features of the tree (height, diameter, shape, bark, acorns, leaves); color, grain and appearance of the wood; structure of the wood; representative uses of the wood; weight of seasoned wood (in pounds per cubic foot); modulus of elasticity; modulus of rupture; remarks (the latter general in nature). Each topic is given a paragraph, and each species of wood is given one page, and no more. The book is, therefore, a very handy one for reference, since all that is said about any particular wood is seen at a glance on one page. Thirty-nine excellent 'half-tone' plates add much to the usefulness of the volume. It is not too much to say that this book should find a place in every botanical library, and unless we are much mistaken, it will soon become an indispensable work in the hands of those to whom it appeals more directly, and for whom it was primarily designed.

### ANOTHER MOUNTAIN LABORATORY.

For several years the University of Montana has maintained a biological station and laboratory at Flathead Lake, Montana. This year it offers its fifth annual session, and there will be opportunities for study in botany, zoology, entomology, nature study and photography. The work in botany includes field study and collecting, classification, type forms, structure, methods of preservation, etc. A general course in ecology and local plant geography is offered also. The region is one which offers opportunities for work on many botanical problems. The surface of the lake is over four thousand feet above sea level, and is surrounded by mountains reaching an altitude of ten thousand feet. It should attract many students of nature.

### SPECIMENS OF FUNGI.

Dr. E. S. Salmon, of Charlton House, Kew, England, the well-known student of the powdery mildews (Erysiphaceæ) desires American students of fungi to procure for him specimens of the fruiting stage of *Erysiphe graminis* occurring on *Poa* and other related grasses. The conidial stage is quite common, but the fruiting stage is less so, and it is the latter alone which Dr. Salmon desires. American collectors should see that he is supplied with an abundance of good material.

Charles E. Bessey.
The University of Nebraska.

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## *SCIENTIFIC NOTES AND NEWS.*

The degree of LL.D. was conferred last week on a number of American men of science, as follows: Harvard University, Professor E. C. Pickering, director of the observatory; N. S. Shaler, professor of geology; William James, professor of philosophy. Yale University, William H. Brewer, professor emeritus in the Sheffield Scientific School. Dartmouth College, E. L. Nichols, who has resigned the chair of physics at Dartmouth to accept a similar position at Columbia University; Alfred Thayer Mahan, U.S.N. Amherst College, Frederick J. E. Woodbridge, professor of philosophy in Columbia University, who graduated from Am-

herst College in 1889. Williams College, Dr. Henry S. Pritchett, president of the Massachusetts Institute of Technology; Dr. E. A. Birge, professor of zoology in the University of Wisconsin, who graduated from Williams College in 1873. Wooster University, Dr. J. H. Hyslop, formerly professor of philosophy in Columbia University. The University of Illinois has conferred the degree of Doctor of Engineering on S. W. Stratton, chief of the Bureau of Standards, and on Professor Ira O. Baker, of the university; it has conferred the degree of Doctor of Agriculture on Professor T. F. Hunt, of the Ohio State University.

PRESIDENT IRA REMSEN, of the Johns Hopkins University, gave the commencement address at Mount Holyoke College on June 24.

PROFESSOR JAMES M. CRAFTS has resigned his position as one of the trustees of the Elizabeth Thompson Science Fund, and Professor Theodore W. Richards has been elected to fill the vacancy.

MR. W. W. STOCKBERGER, instructor in botany in Denison University, has resigned to accept a post as special research assistant in the Department of Agriculture, Washington, D. C.

R. S. LEA, who has been professor of civil engineering and lecturer in mathematics in the faculty of applied science at McGill University during the past eleven years, has resigned to devote himself to professional pursuits.

DR. WILFRED NEWSOME STULL has been appointed Carnegie research assistant to Professor Theodore W. Richards, at Harvard University.

THE Stevens' Triennial Prize of Columbia University for original research has been awarded to Drs. L. Pierce Clark and Thomas P. Prout, of New York, for their 'Status Epilepticus: A Clinical and Pathological Study of Epilepsy.'

MME. SKLODOWSKA CURIE has received the degree of Doctor of Science from the University of Paris, her thesis being based on her well-known researches on radio-active substances.

A PRESS despatch from Tacoma reports that the expedition which recently started north on the Fish Commission steamer *Albatross*, under the leadership of Dr. David Starr Jordan, president of Stanford University, has been forced to return after the vessel had reached Fort Rupert on her way to Arctic waters, where conditions of fish and seal life were to have been investigated. The return was due to the discovery of smallpox aboard, the disease afflicting one of the crew. The vessel will be cleaned and fumigated and the party held for observation for nine days.

PROFESSOR CHARLES E. BESSEY, of the University of Nebraska, was given a short leave of absence by the regents of the university at a recent meeting. He intends to sail for Europe early in July, going directly to Germany, where he is to be joined by his son Ernst A. Bessey, and together they are to proceed to southeastern Russia, in the Caucasus region. Here they are to investigate the vegetation on both sides of the mountain range, returning by way of the Black Sea, on whose northern coast they intend to work for some time. Professor Bessey intends to return to America late in the autumn.

W. D. HALLIBURTON, M.D., F.R.S., professor of physiology, King's College, London, has accepted an invitation extended by the faculty of the University and Bellevue Hospital Medical College to deliver a course of twelve lectures on pathological chemistry, beginning on Monday, January 4, and continuing daily until January 16, 1904. The funds are provided from an endowment given by the retiring professor of pathological chemistry, Dr. Christian A. Herter. Dr. Herter has also endowed a research fellowship in pathological chemistry. Mr. Louis C. Tiffany has furnished a fund for a research fellow in physiology for the ensuing year.

CHANGES in the stations and duties of officers of the Signal Service have been ordered as follows: Colonel H. H. C. Dunwoody, relieved from duty at headquarters, Department of the East, and ordered to duty at Fort

Myer, Virginia; Lieutenant-Colonel James Allen, on completion of present duty, will take station in this city as signal officer of the Department of the East; Lieutenant-Colonel Richard E. Thomson is relieved from duty at Fort Myer, and ordered to Vancouver Barracks, Washington, for duty in the Department of the Columbia, in addition to which he will assume charge of the military system in Alaska; Captain George C. Burnell, at Juneau, Alaska, has been ordered to Seattle, Wash., where he will assume charge of the cable system between Puget Sound and Alaska.

Professor William Elder, of the department of chemistry at Colby University, died on June 25. He was born in Nova Scotia about sixty years ago. He graduated at the Provincial Normal School in 1860 and at Acadia College in 1868. The next year he studied at Harvard under Professors Agassiz, Cooke and Shaler. From 1869 to 1873 he was professor of physical sciences at Acadia College. In 1873 he was called to the professorship of chemistry and natural history at Colby University and remained at the head of that double department till 1885, when he became Merrill professor of chemistry.

The United States Civil Service Commission announces that, owing to the small number of applications filed for the examination scheduled for June 29–30 for the position of teacher in the Philippine Service, the examination will be postponed to July 22–23. The salary at first is from $900 to $1,200 with opportunity for advancement. There are now about 850 American teachers in the Philippine service. The subjects of the examination include the ordinary branches taught in normal schools, and optional subjects may be taken, including the sciences. It seems unfortunate, and may perhaps account for the small number of applications, that the result should be based almost exclusively on an elementary written examination, eighteen points being given for this, and only two points for experience, training and fitness.

The International Institute of Sociology holds its fifth congress this year in the Sorbonne, at Paris, from July 6 to 9, under the presidency of Dr. Lester F. Ward, of Washington. The leading subject for discussion will be the relation of Sociology to Psychology. The Institute was founded in 1894 with Sir John Lubbock (Lord Avebury) as its first president. It is thoroughly international and each of the presidents annually chosen represents a different country. Dr. Ward is its first American president. It has published nine volumes of *Annals,* in which a great variety of subjects relating to the social sciences are discussed from a strictly scientific point of view.

A telegram has been received at the Harvard College Observatory from Professor Kreutz, at Kiel, stating that a comet was discovered by Borelly at Marseilles, June 21, 469, G. M. T. in R. A. $21^h\,52^m\,52^s$ and Dec. $-8°\,10'$. The comet had a daily motion of $-7'$ in R. A. and $+44'$ in Dec. Nucleus and tail were observed.

We learn from the report in the London *Times* that there was a very large audience at the Royal Institution on June 19 to hear Professor Pierre Curie, of the Sorbonne, Paris, lecture on radium. Sir William Crookes was in the chair, and among those present were Mme. Curie, Lord Kelvin, Lord Rayleigh, Lord Avebury, Sir Frederick Bramwell, Sir Oliver Lodge, Professor Dewar, Professor Ray Lankester, Professor Ayrton, Professor S. P. Thompson and Professor Armstrong. Professor Curie, who spoke in French, began with some experiments to illustrate the properties of radium. He showed that it was capable of spontaneously and continuously disengaging heat, that it had the power of rapidly affecting photographic plates even through opaque bodies, and that it could provoke luminous phenomena in phosphorescent substances such as platinocyanide of barium, not losing its power even when cooled to the temperature of liquid air. He next proved its ability to render air a conductor of electricity, by showing that a charged electroscope was at once discharged when a frag-

ment of radium was brought into its vicinity, and in another experiment showed that it facilitated the passage of the electric spark. He went on to describe the different radiations given off by the substance and to distinguish them according to their power of penetration, absorptibility, behavior in a magnetic field, etc. He then explained that, in addition to these radiations, radium also gave off emanations which had the same properties as the substance itself—properties which were included in the term radioactivity. The salts of radium in solution gave off this radioactivity, and were able to render other objects of all sorts radioactive. In this emanation, for example, a charged electroscope was discharged, and a phosphorescent substance became luminous. The emanation behaved in many ways like a gas. It could be aspirated through a tube, it could be condensed by liquid air, and after being frozen out of a vessel would diffuse throughout it again when the temperature was allowed to rise. These phenomena were illustrated by a very pretty experiment, in which a vessel containing a weak solution of radium chloride was connected by a tube to another vessel containing some sulphide of zinc. So long as the stopcock on the tube connecting the two vessels was closed the sulphide of zinc did not phosphoresce, but as soon as it was opened the luminous effect appeared. Returning to the heat disengaged by radium, the lecturer proved the reality of the phenomenon by the aid of what he said was in fact a liquid air calorimeter. A small piece of glass was lowered into a carefully isolated vacuum-flask containing liquid air, and the amount of gas that boiled off in a given time was measured. The experiment was then repeated, but instead of the plain piece of glass a small vessel, identical in size, containing radium was substituted, with the result that in the same time the quantity of gas given off was seen to be more than doubled. Professor Curie concluded with a slight reference to some other properties of radium, its chemical effects, its place in the periodic table of the elements, its power of producing sores on the skin and even of inducing paralysis, and the character of its spectrum. He also gave a brief account of the studies which led Mme. Curie and himself to the recognition of it and other radioactive bodies, and touched on the speculations suggested by the phenomena it presented as to the evolution of matter and the gradual transformation of the elements.

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## *UNIVERSITY AND EDUCATIONAL NEWS.*

THE state appropriations for the University of Illinois this year reached the wholly unprecedented sum of $1,260,000. The sum of $150,000 was given for enlarging the engineering equipment. The College of Agriculture received $100,000 for equipment and instructional work, and the experiment station associated with the college received $170,000 for research work. The ordinary operating fund of the university was increased about $100,000 per year which will make this fund about $350,000 per year. The library fund was doubled, being made $20,000 per year. The sum of $80,000 was voted for a Woman's Building. The sum of $14,400 was given for the maintenance of the department of commerce. The smaller appropriations included $10,000 for cabinets, collections, apparatus, etc., and $10,000 for equipping the chemical laboratory.

AT the eighty-sixth commencement of Hamilton College, President Stryker announced a gift of $100,000 in U. S. Steel Corporation bonds from Mr. Andrew Carnegie, in recognition of the public service of Secretary Root, a graduate of Hamilton. Mr. Carnegie has also given $50,000 to Beloit College for a library building.

COLBY COLLEGE has received gifts amounting to $46,000, including $20,000 from the estate of S. S. Smith, D.D., the author of the hymn 'America,' and formerly trustee and professor of the college.

The board of regents have completed arrangements for creating a school of applied science in the State University of Iowa. A professorship in electrical and mechanical engineering was created. Professor C. S.

Magowan was elected professor of municipal and sanitary engineering—that chair being created. He will also be professor in charge of drawing in connection with civil engineering. Professor A. G. Smith was elected professor of mechanics in the department of mathematical engineering.

The board of regents of the State University of Iowa during their recent session passed resolutions instructing the university architect to report plans at the September meeting for the erection of a building designed to give relief to the present crowded condition of the University Museum and Library. The pian which at present seems to meet with most favor is the erection of one wing of a large building that is to be ultimately used for a museum purpose only, but which would in part be used at first also for the accommodation of the general library, department libraries remaining for the time where these now are. The museum would at once obtain permanent quarters as far as the space thus provided would permit. By vacating the third floor of the present science building, through removal of the museum, space would be secured for the better accommodation of the remaining biological departments.

At the annual commencement exercises of the University of Nebraska on June 11, degrees were conferred as follows: Bachelor of Arts, 132; Bachelor of Science, 38; Bachelor of Law, 84; Master of Arts, 6; Doctor of Philosophy, 2. No honorary degrees were conferred. Of the Bachelor of Science, four received the degree on the completion of the course in civil engineering, seven in electrical engineering, and three in mechanical engineering, the remainder having completed the general science course. Of the six Masters of Arts two, George T. Hargitt and Erle M. Stevenson were in zoology, one, Samuel R. Williams, in physics, and one, John W. Hilton, in philosophy. One of the Doctors of Philosophy, John Lewis Sheldon, took the degree in botany.

At the June meeting of the board of trustees of the University of Illinois, the following new appointments were made: Oscar Adolph Leutwiler, assistant professor of machine design; Dwight T. Randall, assistant professor of steam engineering; Banus Hutson Prater, instructor in civil engineering; Amos William Peters, instructor in zoology; John Henri Walton, instructor in chemistry; John James Harman and Robert Hayden Kuss, instructors in mechanical engineering; Lester Abram Waterbury, instructor in civil engineering; William F. Schultz, instructor in physics; Edward O. Heuse and Edna D. Hoff, assistants in chemistry; Emery Roe Hayhurst, assistant in physiology; William S. Bullard, assistant in zoology; Ira Obed Schaub, assistant in chemistry in Experiment Station; Clifford Willis, assistant in soil physics.

Professor Forest R. Jones, of the Worcester Polytechnic Institute of Technology, has received the appointment of professor of machine design at Cornell University.

Professor A. Ross Hill, of the University of Nebraska has been elected professor of philosophy in the University of Missouri, and Professor F. C. French, of Colgate University, has been elected to the chair at the Universty of Nebraska.

Professor A. E. Taylor, M.A., of Owens College, Manchester, has been appointed to the John Frothingham chair of philosophy at McGill University.

A. D. Sorrensen has been appointed associate professor of psychology and moral philosophy at Colby College.

Dr. B. S. Merigold, instructor in industrial chemistry at the Massachusetts Institute of Technology, has been elected assistant professor of chemistry at Clark College.

George A. Hanford, Ph.D. (Yale), will next year have charge of the department of chemistry at the Medical School of Syracuse University.

Mr. Percy Elford has been elected secretary of the Technical Instruction Committee for the county of Oxford, at a salary of £600 a year. He retains his lectureship of chemistry at St. John's College, Oxford.